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ON

HYGROLOGY, HYGROMETRY,

AND

THEIR CONNEXION

WITH THE

Phenomena observed in the Atmosphere.

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1812.

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*Phenomena observed in the Atmosphere**.

IN the third part of my paper on the *electric column*, published in your Number 124, for December, 1810, where I have considered that instrument as an *aerial electroscope*, I have shown the importance of studying all the *atmospheric* phenomena, before a final decision could be obtained of the question agitated for some time, on the *nature* of *water*; whether it is a *compound* or a *simple* substance; a question which embraces the whole *theory of chemistry*. Especially I hope to have made it evident in that paper, that, since *atmospheric* phenomena are to be considered in the solution of the above question, we ought to study particularly all those of the *electric fluid* in the atmosphere; to which we might be led by the phenomena of the *aerial electroscope*, provided we did not connect them with arbitrary hypotheses, nor forget to take into consideration the *nature* of the *electric fluid*, which, from the great phenomena of *lightning* and *thunder*, has evidently a great share in *meteorological* appearances. My papers, Sir,

Atmospheric phenomena important to the knowledge of the nature of water:

and particularly those connected with electricity.

* From Nicholson's Philosophical Journal, vol. XXXIII.

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in the last numbers of your Journal, were destined to show, from our own experiments and atmospherical observations, what are the *nature* of the *electric fluid*, and its interference in meteorological phenomena; and I now come again to the same subject, under another point of view.

Rain not from moisture in the air:

1. My observations of the *aerial electroscope*, published in your No. 124, show, that the changes in the phenomena exhibited by this instrument have no connexion with the state of *moisture* in the *ambient air*. I proved also, in the same paper, this important point in meteorology, that *rain* does not proceed from a *moisture* actually existing in the atmosphere. This, if it be certain, overturns the new *theory of chemistry*; for thus *rain* cannot proceed from any other cause than that of a *decomposition* of the *atmospheric air* itself, a fluid *sui generis*, the *ponderable* part of which must be *water*.

but the ponderable part of the air itself.

Grounds of the conclusion.

2. But this conclusion rested on the indications of the *hygrometer*, Mr. De Saussure's observations, and my own, on high mountains; in the very region of the atmosphere we saw the *clouds* forming around us, and pouring *rain*, while an instant before our *hygrometers* testified, that there was very little *moisture* in the *air*. But here a question arises: is the *hygrometer* an instrument to be depended upon, for the purpose of indicating the real quantity of *moisture*, or *evaporated water*, mixed with the *air*, in the place where it is observed?

Can the hygrometer be depended on?

This an important question.

3. This, Sir, is a very important question, as well in natural, as in experimental philosophy; and I wish, through your valuable Journal, to attract the attention of your readers to this instrument. I had very little hope of success on this point, when I wrote my preceding papers in your Journal; because, from a circumstance which I shall explain hereafter, none of my *hygrometers* could be found; but it is not the case now.

Progress made in the inquiry.

4. I had already made some progress in the correspondent researches of the indications of the *hygrometer*, and the phenomena of *rain* and *fair weather*, when, in 1786, I published in London my work, *Idées sur la Météorologie**; but I had carried them much farther, when I delivered to the Royal Society my papers on *hygrology* and *hygrometry*, published

* This work may be had of Messrs. Dulau and Co. booksellers in Soho Square.

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in the *Phil. Transactions* for 1790 and 1791 ; the subjects of which I shall here shortly explain, for those of your readers who do not possess the *Phil. Trans.*

5. There is no physical instrument, the name of which terminates in *meter*, as used for *measuring* the *intensity* of the *cause* acting upon it, so deserving that name, as the *hygrometer* described in these papers; for this instrument alone has the property of measuring the *whole extent* of the cause which influences it; which *extent* is comprised between two natural and opposite *extreme points*, one of which I shall first describe : it is *extreme dryness*, or absence of all *moisture*; which, therefore, is an absolute 0. I have proved, in the above papers to the Royal Society, that this point is effectually obtained, by placing the *hygrometer* in a close vessel, filled previously with a sufficient quantity of fresh calcined *lime*, taken *red-hot* from the kiln.

The hygrometer a perfect measurer of moisture in the air.

Point of extreme dryness.

6. The principle which led me to this method is, that, *evaporation* being produced by *heat*, if *red-heat* is not destructive of a *hygroscopic body*, it must occasion the *evaporation* of all the *uncombined water* the latter contains in its pores. And by previous experiments on various bodies of that kind, I found, that *lime*, passing from *red-heat* to *extreme moisture*, increased in proportion of nearly *half* its *weight*. I fixed therefore, upon *lime*, and I employed a large vessel, which I filled with *red-hot lime*. When it was cool, that vessel having at the top small openings for introducing the *hygrometers*, (after which they were closed, and opened only for taking them out,) I took thus the point 0 on a great number of various sorts of *hygrometers*, of which I shall speak hereafter. I have described this vessel in the Phil. Transactions; it is cylindrical, 1 foot diameter, and 3 feet high ; I have it still, and when I place in it one of the *hygrometers*, the 0 of which had been fixed in it 10 years ago, I do not find any sensible difference in this point. Thus, therefore, the point of *extreme dryness* is perfectly ascertained.

Principle on which the ascertaining it is founded.

Not varied by time.

7. As to the opposite point, that of *extreme moisture*, I have proved in the same paper, that it was surely obtained by immersing the *hygrometer* into *water*; where it soon attains a point, beyond which it does not go, whatever length of time it remains

Point of extreme moisture.

Scale.

remains there. This point I have called 100, and the scale is divided into 100 parts.

Inquiry after the fittest substance for hygrometers.

8. Another important object treated in the same paper, and which occasioned me much labour, was, of what substance the *hygrometer* should be constructed. On this particular point I related a long series of experiments, occasioned by the first results I obtained by trying many kinds of *animal* and *vegetable* substances: some of which could be used in thin *threads*, torn in the *length* of their *fibres*; and also in thin *slips* cut *across* the *fibres*. Now, I found, that when used in the *length* of the *fibres*, their *lengthening* by *moisture decreased*, and at last they were even *shortened*, while the same substances cut *across* the fibres continued to *lengthen*: which at first embarrassed me very much*.

Exp. to find whether substances cut lengthwise imbibed moisture while shortening.

9. I could not decide immediately from these observations, whether the substances taken in *length* continued to imbibe *moisture*, while, however, their *length* was *decreasing*; and in order to ascertain this necessary point, I contrived a vessel, described in the same paper. In that vessel I enclosed together several pairs of *hygrometers*, made of the same substances; in one, it was used in the *length*, and in the other *across* the *fibres*; and a *beam*, indicating the 500th part of a grain, to which I

Reason why the substance should be cut across the grain.

* The reason of the difference in the successive expansion by moisture of the same fibrous substances, taken in the *length* and *across* their *fibres*, proceeds from the nature of these substances. The main *fibres* in their *length* are united by *fibrils*, which are seen when we split these bodies. These small *fibres* form with the larger ones a sort of *meshes*, similar to those of a *net*. The first effect of *moisture* is on the *longitudinal* fibres, which it *lengthens*; but when it penetrates the *meshes*, it *widens* them, and thus *shortens* the body; as the *length* of a *net* is *lessened* by stretching it *across*. *Moisture* therefore acts in two opposite ways on the *fibrous substances* taken *in length*, differently in its progress on the same substance, and differently also in different substances. And besides, the whole lengthening is very small in all of them. Now, one of these effects is suppressed by taking the same substances *across* the *fibres*, namely, that which acts on the length of the latter; there remains only that which acts on the *breadth* of the *meshes*, which, if not absolutely proportional to the increase of *moisture*, is never in an opposite sense. Besides, there is a great gain with respect to the extent of the lengthening, and therefore of the *degrees* of the *hygrometer*; for instance, a *slip of whalebone*, by passing from *extreme dryness* to *extreme moisture*, increases nearly *one ninth* in *length*.

suspended

suspended very thin *shavings* of the same substances as the enclosed *hygrometers*; which *shavings* indicated, by the increase of their *weight*, the *weight* of the *water* which penetrated them. I had a *lime-vessel* by which I first produced *extreme dryness* in the vessel containing the instruments; and when I had observed them in that state, and taken off the vessel containing the lime, I had also a manner of increasing *moisture* by degrees in that of the instruments, observing at each step the motions of the *hygrometers*, and the increase of weight of the *shavings*.

10. The general results of this experiment were the following:—1. That substances taken in *length* continue to imbibe *moisture*, though they cease to *lengthen*, and some even begin to *shorten*. 2. That *slips* cut *across* the *fibres* continue to *lengthen* so long as the *moisture* increases. 3. That the *slip* of *whalebone* follows very nearly in its *lengthening* the rate of the increase of *moisture*, indicated by the increase of *weight* in its *shavings*. From this last result, and from the great elasticity of this substance, which makes it always sensibly return to the same *length* with the same *degree* of *moisture*, I fixed on a *slip* of *whalebone* for my *hygrometer*. Results. Whalebone preferable.

11. Such was the point which I had attained, when I delivered my papers to the Royal Society; thus concluded by the determination of an absolute and comparable *hygrometer*, which was wanting in the set of meteorological instruments commonly observed: but by an unlucky circumstance, it still remains little known, and thus enters very seldom into the considerations concerning meteorological systems. I had directed, in the construction of that instrument, a very able German instrument-maker in London, Mr. Haas; but after he had sold a few, he was engaged to go to Portugal, with a pension from the government; and since that time, no other instrument-maker had undertaken to construct it. But lately a Hanoverian gentleman, Mr. Hausmann, who lives now at Cumberland lodge, near Windsor, seeing that it was a very important instrument for meteorology, has undertaken its construction, and having succeeded, he is disposed to make it for those experimental philosophers, who may wish to have it. The instrument little known: but now may be bought.

12. So far, however, as may be seen in the above account of these experiments, I had only obtained a *ratio* between the The quantity of water in air answering to

quantities

given degrees of the hygrometer still remained to be found.

quantities of *moisture*, and the *degrees* of my *hygrometer*; or what *part* each *degree* was of the *whole*: but I had not obtained a knowledge of the *absolute* quantity of *evaporated water*, which, in a given bulk of *air*, corresponded to these *degrees*; a knowledge very essential in the investigation of the *cause* of rain. I saw that this was at least necessary for obtaining more certainty in meteorological conclusions. I relied in this respect on Mr. De Saussure's experiments, as I had not yet had time to undertake them myself; but I thought then to repeat the same experiments, for the following reasons.

Mr. De Saussure's experiments objected to.

13. Mr. De Saussure had made these experiments with his *hair-hygrometer*, which was so dissimilar to mine in the rate of *lengthening* with the same *increases* of *moisture*, that his results could not be immediately applied to my instrument. But especially, he had made all these observations in the course of one day; so that he could only obtain a few immediate points of comparison, whence he deduced a general law of the correspondence of the *degrees* of his *hygrometer* with the *quantities* of *evaporated water* in a given *bulk of air*. This was a first reason why some natural philosophers did not admit the results of his experiments. There were also some other reasons, which I shall hereafter mention: but these results were so important in meteorology, as he himself explained, that I resolved to repeat the same experiments in such a manner, as to remove all the objections, which I clearly saw could only affect the exactness of his experiments, but not their main results. I shall now mention all these objections, and the manner in which I proposed to remove them.

The author resolved to repeat them.

1st. objection.

14. The first objection, as I have said above, was the short time employed in his experiments, to which he had been obliged by the nature of his vessel: I therefore wanted to use a vessel in which I could prolong these operations as long as I should find it necessary. A second objection had been made against the manner in which he first produced *extreme dryness* in his vessel, which was by new-calcined *salt of tartar*; a substance which has *chemical affinities* with *water*, and might absorb *air* with it: I wanted therefore to use new-calcined *lime*, as I had used it for fixing the point of *extreme dryness* on my *hygrometers*. Lastly, there was an objection against the manner by which he had determined the *quantities of evaporated water*

2nd. objection.

3d. objection.

water in his vessel: it certainly could not be very exact; but it was sufficiently so, for the final and most important conclusions of a first attempt of these experiments. However, these objections had rendered the greatest number of experimental philosophers inattentive to this great step concerning meteorology, so that it was almost forgotten. This was my first motive for undertaking the same experiments with the precautions above explained.

15. I found this attempt much more difficult than I had expected; for it cost me more than two years in useless trials, for obtaining, first, a vessel which would remain *air-tight* during all the time that these experiments should require. At last, however, I succeeded, and the experiments themselves took me afterward more than one year. These experiments are related in a work which I published at Paris, in 1803, under the title of *Traité élémentaire sur les Fluides expansiblés*: but on account of the present circumstances of Europe, and this work being in French, a few copies only are come to England. This, Sir, makes me desirous to consign to your Journal a short account of these experiments.

The experiments difficult.

Related in a French work.

16. My purpose was to ascertain what quantities of *evaporated water* in a known *space* of *air* corresponded to each *degree* of my *hygrometer*; and I determined, that this *space* should be one *cubic foot*. My first success in overcoming the difficulties was that of obtaining a vessel, which would remain *air-tight* during the whole course of these experiments. I found, that no vessel could be rendered air-tight so long, which had a large opening at the top; and that therefore this opening should be only what was necessary to introduce the instruments into it. I then procured a *glass vessel*, about 23 inches high, and $8\frac{1}{2}$ in diameter, the opening of which at the top was only $2\frac{1}{2}$ inches in diameter. I measured the *capacity* of this vessel; it was not quite one *cubic foot*; but I ascertained the differences to which I was to proportionate the quantities of *evaporated water*, so that they might be as 1 *grain* in a *cubic foot*.

Object of the experiments.

Vessel for making them in.

17. Before that time, I had found a sure method of ascertaining the quantities of *water* successively evaporated in a vessel, without opening it; in order to prevent any exchange of the internal with the external air, lest the latter should introduce

Method of ascertaining the quantity of water evaporated in a vessel with certainty.

duce some moisture with it. This method was to enclose equal quantities of *water* in very thin and small *glass bubbles,* with a neck drawn to a very small point, easily sealed with the flame of a taper; and before this last operation, I determined the quantity of *water* that each contained, by a beam which indicated 1000th part of a *grain.* These *glass bubbles* were placed in the upper part of the vessel on a circular stand, and I had, outwards at the top, a mechanism for breaking them without opening the vessel. This method I applied to the glass vessel above mentioned.

Mr. de Saussure showed, that rain is not condensed by cold.

18. Such were the means which I employed for ascertaining the quantities of *evaporated water* in a *cubic foot* of air, acting on the enclosed *hygrometer.* But these experiments required another condition, which Mr. de Saussure had already introduced in them: because those natural philosophers, who attributed *rain* to the *moisture* in the atmosphere, had supposed, that this *moisture* was condensated by *cold.* Mr. De Saussure had sufficiently proved, that it was not the case, by observing the effects of the changes of *temperature* on his enclosed *hygrometer.* I was therefore to introduce the same condition in my experiment, and for this purpose I enclosed also in my vessel a *thermometer* with Fahrenheit's scale. Lastly, as I intended to make the same observations on every successive *grain* of *evaporated water,* which would take a very long time; having previously found that *extreme moisture* was produced in the vessel by a small number of *grains* of *water*; and even that they could not undergo great changes in the degree of *heat,* without some *water* being deposited in the sides of the vessel: this obliged me, in order to obtain the same temperatures in the observations of the effects of each successive *grain* of evaporated water in the vessel, to make these experiments only in the spring and the autumn; because, in these seasons, I could obtain naturally almost every day in my room the temperatures of 50, 55, 60 of Fahrenheit, on which I fixed for all these experiments. By this method I was sure, that the temperature would be always the same in every part of the vessel it being that of the air in the room.

Thermometer enclosed in the vessel.

Extreme moisture produced by a few grains of water to a cubic foot of air.

The experiments confined to spring and autumn for uniformity of temperature.

Two series of experiments made.

19. I made two series of these experiments; one beginning in the autumn of 1795, and ending in January, 1796; the other beginning in the autumn of 1796, and terminating in February,

February, 1797: each of them began by producing *extreme dryness* in the vessel, and proceeded by the *evaporation* of successive grains of water; observing afterward the changes produced on the *hygrometer* at the three fixed *temperatures*. In the course of these experiments I had a proof, that the vessel remained *air-tight*. For in order to ascertain the effects of the increase of water at the three temperatures, I consecrated many days, even weeks, to the observation of each step, by repeating it many times; which made both sets of experiments last near 6 months: however, I found no sensible difference in these observations from the first to the last day, with every quantity of water; and in ending them, I had an immediate proof, which it would be too long here to explain, that the *aqueous vapour*, which had been produced in the vessels, had added its expansibility to that of the *air* originally enclosed in it.

The vessel remained air-tight.

The experiments many times repeated.

20. This, I think, was a complete determination of the correspondence between the *degrees* of my *hygrometer*, and the *quantities* of *evaporated water* in one *cubic foot* of air, at the observed degrees of *heat*. I then undertook to derive from these experiments general rules of *hygrometry*. These deductions begin at p. 325, of the 2d vol. of the above-mentioned work; they are given in 13 successive tables, of which I shall only mention two.

Rules of hygrometry deduced from the experiments.

21. In *table* ii. are united the results of both experiments, (which differ very little from each other), reduced to their mean terms. Each set began at the point of *extreme dryness* in the vessel; a point where the *hygrometer* stood at 0 in both. At that point, no *moisture* being in the vessel, the change of *heat* from 50 to 60 of Fahr. produced no change in the *hygrometer*. During both sets of experiments, the limits of the *evaporation* in the vessel were the same: 5 *grains* only of *water* could remain *evaporated* at the temperature of 50; 6 *grains* at that of 55, and 7 *grains* at 60. Beyond these quantities, at the respective *temperatures*, a certain quantity of *water* was deposited on the sides of the vessel in the form of *dew*; but when this effect took place at the temperature of 50, the *dew* was dissipated when the *heat* of my room came to 55; and when it happened at 55, it was dissipated when the *heat* in the room arrived at 60.

Limits of evaporation at different temperatures.

22. Thus therefore we have the natural *limits* of the quanti-

ties

ties of *evaporated water* that can subsist in one *cubic foot* of *air* with these three degrees of heat; but by the rate of its progress, this correspondence may be continued to higher and lower temperatures, as I shall explain, after the following indication of the immediate effects observed on the *hygrometer* of each increase of 1 *grain* at the three *temperatures*. In the first two columns of the table, the points of the *hygrometer* cease to be indicated at the period when *dew* appeared on the side of the vessel.

Table of moisture indicated by the hygrometer at different temperatures.

Grs. of water in 1 cubic foot.	Points of the hygr. at temp. 50°.	Points of the hygr. at temp. 55°.	Points of the hygr. at temp. 60°.
	°	°	°
1	15·2	14·5	13·9
2	29·9	28·5	27·6
3	51·6	47·2	43·2
4	74·9	64·1	55·
5	89·8	78·6	68·3
6		93·9	82·1
7			96·6

Remarks on this table.

23. This table shows the progress of the effects on the *hygrometer* of the *evaporation* of the successive *grains* of water. These increases were stopped, as I have said above, by some water being deposited on the sides of the vessel. This effect took place for the 6th *grain* with the temperature 50°, and for the 7th *grain* at 55°: however, this happened only when the *grains* were entirely evaporated, during which time the *hygrometer* had moved; but there was no fixed point to be obtained correspondent to the new *grain* of *water*, since a part of it at last was deposited on the sides of the vessel.

Account of the other tables in the work.

24. The tables which follow this, in my work, serve to combine these results, by the rules of interpolation, for obtaining the intermediate terms not given by the experiments; and also to continue the same series, on one side, up to 98 of the *thermometer*, and on the other, for a particular purpose, down to 0. The table ix., which is the result of all these combinations, is constructed in such a manner, as to afford immediately the answer to the following questions, very important in meteorology.

Questions an-

I. A point having been observed on the *hygrometer* in the open

open air, what are the quantities of *evaporated water* in one *cubic foot* of that *air*, at any given *temperature?* swered by table ix.

II. The points of the *hygrometer* and *thermometer* having been observed, what is the quantity of *evaporated water* in one *cubic foot* of that part of the atmosphere?

III. The points of both instruments having been observed, to what degree ought the *thermometer* to fall, in order that the *hygrometer* should arrive in that air at 100; which point it must attain before there is any precipitation of *water?*

No degree of cold in the air can produce clouds and rain.

25. The answers to these questions, from the immediate results of my experiments, led to this first conclusion; that no diminution of *heat* in the atmosphere could occasion in it the precipitation of such a quantity of *water* as to produce *clouds* pouring *rain*; which confirmed me in the opinion already expressed in my work, *Idées sur la Météorologie*, that the *aqueous vapour*, constantly ascending in the atmosphere, ceased in great part to act on the *hygrometer*, being converted into an *aeriform fluid*, namely, the *atmospheric air*, and that *clouds* and *rain* were produced by the decomposition of this *fluid*.

Aqueous vapour ceases to act on the hygrometer from its being converted into atmospheric air.

Conclusions from hygroscopic experiments agreed with the atmospheric phenomena of the Alps.

26. Such was the conclusion of all the above *hygroscopic* experiments; and with respect to *atmospheric* phenomena, it coincided with the observations of Mr. de Saussure and myself in the high regions of the atmosphere. Having both long inhabited our mountainous country near the Alps, we had separately followed the same meteorological observations with our *hygrometers*, and we had absolutely ascertained these two points.—1. That the more we ascend in the atmosphere, the *dryer* the air is observed; and that even, in clear weather, it is *dryer* in the night than in the day. 3. That *clouds, rain, hail,* and *thunder*, are produced in certain *strata* of the atmosphere which were *clear* a moment before, and in which one *cubic foot* of *air* did not contain above *two grains* of *water*. Having both separately, at different times, and also in different parts of the mountains, made the same observations, and published them separately, I cannot suppose, that their results can be contested. Thus it is certain, that *rain* is *not* produced by a *moisture* existing in the atmosphere; and consequently that it proceeds from a *decomposition* of the *air* itself.

The same deductions formed separately by the author and Mr. de Saussure.

27. From what I have said so far, it may be judged, that the Proofs, that

whole

the modern theory of chemistry is erroneous.

whole of this work was intended to prove, how erroneous was the modern *theory of chemistry*, the foundation of which is to suppose, that *water* is a compound of *two* substances, called by its authors *hidrogen* and *oxigen*, and that the *atmosphere* is principally composed of two *fluids* called by them *hidrogen air* and *oxigen air*; a system in which, for the explanation of the greatest atmospheric phenomena, which ought to have been their first objects of comparison, those of *clouds* and *rain*, they had been reduced to suppose a *condensation* of the *aqueous vapour* by *cold*, which supposition the above experiments prove to be absolutely erroneous. This is the only point, which I have here considered; and indeed it is sufficient to overturn the whole theory: but in other parts of the work I entered into the examination of all its parts, beginning with the original experiments from which the *composition* of *water* had been concluded; and in analysing these experiments I made it manifest, that, far from being satisfactory, there were many unwarrantable hypotheses to be made, in order to connect the *facts* with the *conclusion*.

Berthollet's attempt to defend it.

28. When my work had been published at Paris, Mr. BERTHOLLET, one of the authors of that *chemical theory*, attempted, in the *Annales de Chimie*, and in another French Journal, to defend the only resource of that theory, namely, that *rain* was the effect of the *condensation* by *cold* of the *aqueous vapour* existing in the atmosphere. He acknowledged however two points, first, that my experiments with respect to the effects of *evaporated water* on the *hygrometer* at different temperatures had been made with an uncommon accuracy; and that I had thus demonstrated the errour of those, who attributed *evaporation* to a *dissolution* of *water* by *air*. These were two important concessions; but being loth to abandon his theory, and totally unacquainted with meteorological phænomena, he attempted again, as it was absolutely necessary for the support of his theory, to explain *rain* by the *cold* condensating the *aqueous vapour* in the atmosphere; thinking that by transporting the condensation to *very high regions* of the air, no objection could be made from immediate facts: but he was mistaken; since Mr. de Saussure and myself had proved, from immediate facts, that the upper regions of the atmosphere are *dryer* than those that we can attain.

29. I

29. I answered, in the *Annales de Chimie,* to every part of Mr. Berthollet's objections; and neither himself, nor any other experimental philosopher, has ever replied; while, on the contrary, many have abandoned the fundamental part of that theory, the *composition* of *water*: and indeed, one of its first inventors, with whom, having seen his experiments, I had acquiesced in his conclusion, and for a time maintained it, I mean Dr. Priestley, made me himself abandon it, on account of new chemical results obtained in his experiments, which he opposed to Mr. Berthollet.

Answered by the author.

The composition of water given up by one of its first supporters.

30. I have been induced, Sir, to give you this abstract of a work little known in England, in order the more to fix the attention of natural philosophers on the *hygrometer,* of which I have thus proved the importance in natural science. It is difficult to abstain from making *theories* on the first phenomena we observe of a new kind, or from admitting those which appear probable to us; and I have said above, that I had at first acquiesced in that of the *composition* of *water:* but by the progress of experiments, new facts are discovered, and correct the *theories* too soon admitted. My long study of every branch of meteorology, being united with the experiments related in this paper, which indeed were directed to that object, have demonstrated to me this great point in natural philosophy—that it is impossible to attribute *rain* to a *moisture* actually existing in the atmosphere; which alone entirely refutes the new *chemical theory.* Moreover, all the experiments on the combinations of *gasses* with other bodies concur to show, that the *ponderable* part of these fluids is *water.* Lastly, in the above mentioned work I proved, as I have done succinctly, Sir, in my paper published in your Journal for December, 1810, that, when we consider the atmospheric air as an *aeriform fluid,* though never mixed but with a very small quantity of *aqueous vapour,* all the *atmospheric* phenomena are explained.

We are too hasty in forming hypotheses.

I may conclude, therefore, that *meteorology* makes an essential part of *natural philosophy,* and that it is not so obscure as it is commonly thought.

I have the honour to be,

Sir,

Your obedient, humble Servant,

J. A. DE LUC.

Windsor.